PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Is self-esteem associated with self-rated health among French college students? A longitudinal epidemiological study, the i-Share cohort
AUTHORS	Arsandaux, Julie; Michel, Grégory; Tournier, Marie; Tzourio, Christophe; Galéra, Cédric

VERSION 1 - REVIEW

REVIEWER	Oliver Huse Deakin University, Australia
REVIEW RETURNED	20-Aug-2018

GENERAL COMMENTS	Thank you for the opportunity to review this paper, which describes the association between self-esteem and self-rated health in a population of college students. The authors have done well to fill a gap in existing research. However, the authors may have overstated the importance of self-rated health and greater clarity regarding the significance of this work is required. Additionally, many statements are not adequately supported by previous literature (see below for details). Further, the results are not clearly presented and it is difficult to link written results with included table and supplementary tables. I hope that the specific comments below will provide useful feedback to the authors. Introduction 1. Statements such as 'self-rated health may represent a more inclusive and universal predictor than clinical examination, medical records or self-reports of medical conditions' are supported by minimal previous studies of variable quality and breadth. Unless additional references exist, it may be beneficial to adjust the language so as to not overstate the benefits of self-rated health as an indicator of overall health. 2. Self-esteem as an indicator of self-rated health is introduced quite late. It might help the reader if this paragraph (fourth in the introduction) was moved up a bit. Further, it might be useful to explicitly state here why the association between self-esteem and self-rated health is important. 3. The final paragraph of the introduction discusses the i-Share cohort – this would be better placed in the methods. 4. In general, the introduction could do with additional references throughout. Some specific examples include:

	a. Page 5, line 20 to 24 – please outline which studies/disciplines have looked at self-rated health, as currently only reference (9) is included. b. Page 5, paragraph 3 – please provide additional references on college students. It would be very useful to support why this is an important group (line 34/35) and why self-rated health in this group is useful (line 38-44) c. Page 5, line 51 – please provide a reference for 'Rosenberg' d. Page 6, paragraph 3 – please provide a reference for the i-Share cohort here Methods The methods section details the study well, including the analysis performed. However, it would benefit from further detail of the i-Share project. 1. It would be useful for readers to know more about the i-Share project and when data was collected. A timeline/figure would go a long way to detailing this and would help in understanding when specific data were collected (when was time 1, time 2, time 3). 2. It would also be useful to know more about the i-Share cohort including sample size, response rate, mean age, sex, etc. 3. It is unclear why co-variates were taken from a different time to self-esteem or self-rated health. If possible, I suggest instead taking co-variates from Time 3 when data on self-rated health was assessed. If this is not possible, then I suggest including why. Results 1. As with point 2 under the 'methods' heading, please provide some comparison to the full i-Share cohort so the reader can understand how representative your sample is. Additionally, please also give the response rate of the initial sample. 2. In the second paragraph of the results, it is unclear where many of the numbers came from. It seems that some results do not appear in any table and some key results are included in supplementary tables. 3. Please reference supplementary tables in the text so that the reader knows when and why they should be referred to. Additionally, some of the results in these supplementary tables seem to be important – it might be a good idea to move some of them to the main document, specifically elements of Table S2 that are referred to in the text could be added to Table 2 in the main document. Discussion 1. The discussion provides a good overview of the study results and supporting literature. 2. This section would benefit from more detail within the strengths and limitations: a. The authors list a large sample size as a strength, but the study included only a fraction of the original i-Share cohort. Please refer to the study sample in the context of the larger i-Share cohort. b. The limitations of sampling bias might become more apparent if the study population and the total i-Share cohort were compared in the results. If the study sample is no longer representative then the implications for this should be discussed.

	c. The generalisability of the results, given the specific cohort of college students, should be mentioned. Other Comments 1. Some English grammar mistakes can be found throughout the manuscript 2. As with point 4 under the 'introduction' heading, the article would benefit from additional references to support statements.

REVIEWER	Hans Johan Breidablik Dpt. of science and development Helse Førde medical trust Norway
REVIEW RETURNED	06-Nov-2018

GENERAL COMMENTS	Review of : «Association between self-esteem and self-rated health among college students, the i-share cohort» The study looks at the association between self-esteem (SE) and self-ratet health (SRH) among french college students through an internet questionnaire. In this they also look at a broad spectrum of potential counfonders, divided in 6 different areas. They find an adjusted OR of 1,4 (moderat) between postive SE and subsequent SRH, and also a clear association with BMI, economi during childhood and 3 different personality traits. They call this novel findings (which is arguable) and the possibility that interventions aimed at SE could also improve later SRH. This is interesting. But the study har some clear limitations. The most important is connected to selection bias, and therefore the external validity. They are recruiting trough internett, and nearly 80% af the participants are females, 71% are physical active and 90% have a BMI <25. So this is a cohort of mainly halthy female students. Young girls usually have lower SRH than boys. This should be more discussed in the study. The median follow-up time is short (8 months), and around 1000 participants is not much in cohort studies like this. There are many covariates (more than 20?) which is also a challenge in analyses like this. A table over crude (unadjusted) OR for all the covariates should be presented. The OR should also be presented in the same direction (> 1 or <1) to make it easier for the reader. If the study will be published, an external statstican should evaluate the methods and statistics used. Here is more detailed comments one some points: -The publication has 3441 words, and could be preferably be shortened down to maximum 3000 words. -Summary: Prospective design, but short, and moderate number of participants. -Page 5: « …important pitfalls in the litterature». I find the expression pitfall a bit strange in this connection? -Page 5: «only two studies»: I think the authors should seek more in the litterature, se also my suggestions later for references. -Page 6: The part strarting with «The i-Share cohort …» should be moved to another part (introduction/method or omitted). -Page 7: Participant involvement: Could be shortened. -Page 8: Demographic covariates: Why is BMI not rubricated under lifestyle (behavioral) covariates? Results: Table 2: Se also my earlier comment on unadjusted and adjusted OR for all the covariates. Both BMI and financial situation during childhood seems to be stronger associated than SE (OD

	arount 2 (0,5), but the 3 personality traits is close to OR of 1, even if it is significant? This should be discussed more for the reader, and also the implication of this observations? In Table 1 supp mat. We se a great gender difference in univariate model? The same for economic satisfaction at inclusion, family support during childhood? This could be discussed, together with the surprisingly weak association with lifestyle factors. -Page 14: the possibility for «reverse causation» between SE and SRH is noted, this circle or spiral effects is propably important in this area. -Page 15: Implications: Could be shortened. -References: Many (too many?) referances some of them a bit marginal and several years old. -I can suggest the following references from our group for the authors to read: «Self-rated health in adolescence: A multifactorial composite» http://journals.sagepub.com/doi/abs/10.1177/1403494807085306 «Self-rated health during adolescence: stability and predictors of change (Young-HUNT study, Norway)» https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2639013/

REVIEWER	Pavol Mikula, PhD Department of Social and Behavioral Medicine, University of Pavol Jozef Safarik, Kosice, Slovakia
REVIEW RETURNED	04-Dec-2018

GENERAL COMMENTS	Thnak you for the opportunity to read the manuscript titled: Association between self-esteem and self-rated health among college students, the i-share cohort". The manuscript is of good scientific quality. While the rationale and results are not novel, strong methodology and especially longitudinal nature of data are very needed attributes in research focused on psychological variables of self-esteem and self-rated health. Few remarks and questions for authors: Why is median of age reported instead of average. Were there extreme values of age present, that made median better indicator of centrality of the sample? Self-rated health was generally very good in the sample. While this manuscript was aimed at college students, the role of education may be very important and thus I would welcome more information in the discussion concerning self-rated health from the point of view of education. Does it have effect? Does i-share cohort include also people who are not college students? If not, comparison with goup of people with lower education could be very interesting. If the i-share cohort does not include such sample, discussion should contain literature concerning this. While use of BMI is not the prime concern, it should be noted in the limitations that waist measurment is better indicator of physical shape. While people with a lot of muscle may have high BMI, their health can be in better condition compared to lower levels. Coping and interventions regarding self-esteem should be elaborated more as they are crucial implications. Overall, manuscript is good, and what it lacks in novelty, it compensates with high number of participants and good statistical and methodolocigal outputs of robust design.

VERSION 1 – AUTHOR RESPONSE

2. Reviewer: 1

Reviewer Name: Oliver Huse

Institution and Country: Deakin University, Australia

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thank you for the opportunity to review this paper, which describes the association between self-esteem and self-rated health in a population of college students. The authors have done well to fill a gap in existing research. However, the authors may have overstated the importance of self-rated health and greater clarity regarding the significance of this work is required. Additionally, many statements are not adequately supported by previous literature (see below for details). Further, the results are not clearly presented and it is difficult to link written results with included tables and supplementary tables. I hope that the specific comments in the attached document will provide useful feedback to the authors.

Introduction

1. Statements such as ‘self-rated health may represent a more inclusive and universal predictor than clinical examination, medical records or self-reports of medical conditions’ are supported by minimal previous studies of variable quality and breadth. Unless additional references exist, it may be beneficial to adjust the language so as to not overstate the benefits of self-rated health as an indicator of overall health.

In page 7, we have added 3 references and have adjusted the text as follows: “Collection of self-rated health is recommended by the World Health Organization (WHO) as a standard and cost-effective measure in health surveys (4–7). The associations between self-rated health and mortality persist even after objective health adjustment, suggesting that self-rated health could represent an inclusive and universal predictor besides clinical examination, medical records or self-reports of medical conditions (8).”

2. Self-esteem as an indicator of self-rated health is introduced quite late. It might help the reader if this paragraph (fourth in the introduction) was moved up a bit. Further, it might be useful to explicitly state here why the association between self-esteem and self-rated health is important.

We have now followed the reviewer's suggestion by moving up self-esteem in the introduction. In addition, we have explicitly stated why the association between self-esteem and self-rated health is important. See page 8: “Yet, it should be noted that self-esteem is a potentially modifiable factor. If self-esteem is predictive of self-rated health independently of other psychosocial confounders it could then represent a specific target for preventive interventions aiming at improving general health. Interestingly efficient interventions focusing on psychosocial abilities and self-knowledge provide relevant tools to experiment such specific interventions on self-esteem (20-22).”.

3. The final paragraph of the introduction discusses the i-Share cohort – this would be better placed in the methods.

We have now placed the final paragraph of the introduction in the methods (see page 9).

4. In general, the introduction could do with additional references throughout. Some specific examples include:

a) Page 5, line 20 to 24 – please outline which studies/disciplines have looked at self-rated health, as currently only reference (9) is included.

b) Page 5, paragraph 3 – please provide additional references on college students. It would be very useful to support why this is an important group (line 34/35) and why self-rated health in this group is useful (line 38-44)

c) Page 5, line 51 – please provide a reference for 'Rosenberg'

d) Page 6, paragraph 3 – please provide a reference for the i-Share cohort here

As suggested, we have provided additional references throughout the introduction.

Methods

The methods section details the study well, including the analysis performed. However, it would benefit from further detail of the i-Share project.

1. It would be useful for readers to know more about the i-Share project and when data was collected. A timeline/figure would go a long way to detailing this and would help in understanding when specific data were collected (when was time 1, time 2, time 3).

We have provided such a figure entitled "Figure S1. Timeline of data collection in I-Share (supplementary material)"

2. It would also be useful to know more about the i-Share cohort including sample size, response rate, mean age, sex, etc.

We have now added characteristics of the I-Share sample (see page X)

3. It is unclear why co-variates were taken from a different time to self-esteem or self-rated health. If possible, I suggest instead taking co-variates from Time 3 when data on self-rated health was assessed. If this is not possible, then I suggest including why.

Adjusting on co-variates measured at the same time as the outcome should be avoided to keep the precedence of co-variate regarding the outcome, which is a strength of the longitudinal design compared to a cross-sectional design. We have added this statement in the methods. "They were all preceding the outcome to keep the longitudinal sequence between predictors and outcome." (Page 11)

Results

1. As with point 2 under the 'methods' heading, please provide some comparison to the full i-Share cohort so the reader can understand how representative your sample is. Additionally, please also give the response rate of the initial sample.

We have now provided a comparison to the full I-Share cohort (see page X and supplementary material).

2. In the second paragraph of the results, it is unclear where many of the numbers came from. It seems that some results do not appear in any table and some key results are included in supplementary tables. Please reference supplementary tables in the text so that the reader knows when and why they should be referred to. Additionally, some of the results in these supplementary tables seem to be important – it might be a good idea to move some of them to the main document, specifically elements of Table S2 that are referred to in the text could be added to Table 2 in the main document.

As suggested by the reviewer we have now moved some key results to the main tables. We have also referenced the supplementary tables in the text.

Discussion

1. The discussion provides a good overview of the study results and supporting literature.

We thank the reviewer for this encouraging comment.

2. This section would benefit from more detail within the strengths and limitations:

a) The authors list a large sample size as a strength, but the study included only a fraction of the original i-Share cohort. Please refer to the study sample in the context of the larger i-Share cohort.

We have added (page 17): “Although the study sample included only a fraction of the original I-Share cohort, the number of participants was still relatively high compared to the available studies in the area.”

b) The limitations of sampling bias might become more apparent if the study population and the total i-Share cohort were compared in the results. If the study sample is no longer representative then the implications for this should be discussed.

We have mentioned the generalizability issue as suggested (page 17-18): “First, a sampling bias could have arisen since participants were mainly healthy female students, thus limiting the generalization to other student populations.

c) The generalisability of the results, given the specific cohort of college students, should be mentioned.

See above.

Other Comments

1. Some English grammar mistakes can be found throughout the manuscript

We have conducted additional revision of the English writing.

2. As with point 4 under the ‘introduction’ heading, the article would benefit from additional references to support statements.

We have added references when relevant.

3. Reviewer: 2

Reviewer Name: Hans Johan Breidablik

Institution and Country: Dpt. of science and development, Helse Førde medical trust, Norway

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Review of : «Association between self-esteem and self-rated health among college students, the i-share cohort»

The study looks at the association between self-esteem (SE) and self-ratet health (SRH) among french college students through an internet questionnaire. In this they also look at a broad spectrum of potential counfonders, divided in 6 different areas. They find an adjusted OR of 1,4 (moderat) between postive SE and subsequent SRH, and also a clear association with BMI, economi during childhood and 3 different personality traits. They call this novel findings (which is arguable) and the possibility that interventions aimed at SE could also improve later SRH. This is interesting.

But the study har some clear limitations. The most important is connected to selection bias, and therefore the external validity. They are recruiting trough internett, and nearly 80% af the participants are females, 71% are physical active and 90% have a BMI <25. So this is a cohort of mainly halthy female students. Young girls usually have lower SRH than boys. This should be more discussed in the study.

As suggested by the reviewer we have discussed the selection bias (page 17-18) “First, a sampling bias could have arisen since participants were mainly healthy female students, thus limiting the generalization to other student populations. Since young females usually have lower self-reported health than males caution should be taken regarding the extrapolation of the results to males. “

The median follow-up time is short (8 months), and around 1000 participants is not much in cohort studies like this. There are many covariates (more than 20?) which is also a challenge in analyses like this.

As it is now mentioned in the manuscript (see page 17) “Although the study sample included only a fraction of the original I-Share cohort, the number of participants was still relatively high compared to the available studies in the area”.

The number of covariates (>20) is not an issue regarding the main exposure since the choice to keep them in the model was made a priori. Yet it should be acknowledged that caution should be kept regarding the interpretation with respect to the covariates. However, since the significance of the associations regarding most of the covariates is beyond the classical alpha of .05 (most of the associations between covariates and outcome showed a p value < .001), confidence in the results is reasonable.

A table over crude (unadjusted) OR for all the covariates should be presented.

As requested by the reviewer, table S2 presents unadjusted OR for all the covariates.

The OR should also be presented in the same direction (> 1 or <1) to make it easier for the reader.

As suggested, we have now presented OR in the same direction to make it clearer for the readers.

If the study will be published, an external statstican should evaluate the methods and statistics used.

Here is more detailed comments one some points:

-The publication has 3441 words, and could be preferably be shortened down to maximum 3000 words.

If the editor wish so we will further shorten the manuscript.

-Summary: Prospective design, but short, and moderate number of participants.

We have now modified the abstract accordingly

-Page 5: « …important pitfalls in the litterature». I find the expression pitfall a bit strange in this connection?

We have replaced the word “pitfalls” by the word “limitations” (see page 7)

-Page 5: «only two studies»: I think the authors should seek more in the litterature, se also my suggestions later for references.

We have added more references.

-Page 6: The part strarting with «The i-Share cohort …» should be moved to another part (introduction/method or omitted).

As suggested, we have moved this sentence to the method section.

-Page 7: Participant involvement: Could be shortened.

“Participant involvement” as already presented. If the editor agrees we prefer keeping the part the information appears necessary to fully understand the methods. However if the editor wish us to make differently we will follow the editor’s recommendations.

-Page 8: Demographic covariates: Why is BMI not rubricated under lifestyle (behavioral) covariates?

We have now put the variable BMI within lifestyle (behavioral) covariates.

Results: Table 2: Se also my earlier comment on unadjusted and adjusted OR for all the covariates. Both BMI and financial situation during childhood seems to be stronger associated than SE (OD arount 2 (0,5), but the 3 personality traits is close to OR of 1, even if it is significant? This should be discussed more for the reader, and also the implication of this observations?

Since covariates were not the main variables of interest we have decided not to provide further interpretations (see further comment relative to table 1 sup. Mat. bellow). If the editor/reviewer wish so we will however follow their recommendations.

In Table 1 supp mat. We se a great gender difference in univariate model? The same for economic satisfaction at inclusion, family support during childhood? This could be discussed, together with the surprisingly weak association with lifestyle factors.

When interpreting the results of the study the focus is put on the main variable of interest which is self-esteem. The other covariates are adjustment variables. Consequently, we have been cautious when interpreting their associations with the outcomes.

-Page 14: the possibility for «reverse causation» between SE and SRH is noted, this circle or spiral effects is propably important in this area.

We agree with the reviewer's comment. For this reason, this statement was included in the limitations.

-Page 15: Implications: Could be shortened.

We have slightly shortened the section implications.

-References: Many (too many?) referances some of them a bit marginal and several years old.

-I can suggest the following references from our group for the authors to read: «Self-rated health in adolescence: A multifactorial composite»
http://journals.sagepub.com/doi/abs/10.1177/1403494807085306

«Self-rated health during adolescence: stability and predictors of change (Young-HUNT study, Norway)»

https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2639013/

We thank the reviewer for these suggestions. We have now added these references.

4. Reviewer: 3

Reviewer Name: Pavol Mikula, PhD

Institution and Country: Department of Social and Behavioral Medicine, University of Pavol Jozef Safarik, Kosice, Slovakia

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thnak you for the opportunity to read the manuscript titled: Association between self-esteem and self-rated health among college students, the i-share cohort".

The manuscript is of good scientific quality. While the rationale and results are not novel, strong methodology and especially longitudinal nature of data are very needed attributes in research focused on psychological variables of self-esteem and self-rated health.

Few remarks and questions for authors:

Why is median of age reported instead of average. Were there extreme values of age present, that made median better indicator of centrality of the sample?

As suggested, we have replaced the median by the mean (SD).

Self-rated health was generally very good in the sample. While this manuscript was aimed at college students, the role of education may be very important and thus I would welcome more information in the discussion concerning self-rated health from the point of view of education. Does it have effect? Does i-share cohort include also people who are not college students? If not, comparison with goup of people with lower education could be very interesting. If the i-share cohort does not include such sample, discussion should contain literature concerning this.

We agree with the reviewer about the impact of education on self-rated health. The present study exclusively included students. The level of education was consequently homogeneous. It is clear that extrapolation can only be done towards students and not towards people with lower education. In the manuscript only generalization towards students' populations is discussed (see page 18).

While use of BMI is not the prime concern, it should be noted in the limitations that waist measurment is better indicator of physical shape. While people with a lot of muscle may have high BMI, their health can be in better condition compared to lower levels.

In univariate analyses we adjusted on physical activity and nutrition. These factors were not kept in the final models due to their non-significance in the multivariate models.

Coping and interventions regarding self-esteem should be elaborated more as they are crucial implications.

We have elaborated on potential interventions, page 18: “Self-esteem improvement interventions could focus either on (1) global self-esteem by increasing self-knowledge and resilience through an individual intervention, web-based for instance, or (2) a specific dimension such as social self-esteem

by using exercise or mentoring program (19,20).” Due to constraints regarding the number of words we have limited this elaboration. If the editor wish so we can however add elaborate more.

Overall, manuscript is good, and what it lacks in novelty, it compensates with high number of participants and good statistical and methodolocigal outputs of robust design.

VERSION 2 – REVIEW

REVIEWER	Oliver Huse Deakin University, Australia
REVIEW RETURNED	11-Mar-2019

GENERAL COMMENTS	Thank you for the opportunity to review the resubmission of this paper, which describes the association between self-esteem and self-rated health in a population of college students. The authors have done well to address the comments from reviewers. The results could still do with some editing for clarity and the authors should be careful not to overstate the generalisability of their findings. Finally, the paper requires further editing for English language and grammar before it can be considered ready for publication. Introduction The introduction is much improved over the previous submission. As suggested above it would benefit from additional editing for language and grammar. Additionally: 1. The suggestion that self-esteem could present a specific target for interventions is not supported in the text by previous literature. I would suggest that the justification that self-rated health is a good indication general health is sufficient for the introduction (leaving discussion of interventions for the discussion). Methods Again, the authors have incorporated many of the comments into the methods. 1. The paper would still benefit from additional information regarding the i-Share study. In both the text and on the timeline it would be good to know the dates of each time point and the number of participants at each time point (for the i-Share cohort, not this study). a. For example, it seems that neither the methods or results section provide details on follow-up time (but the discussion does, which can be confusing). Results 1. Paragraph one of the results section would benefit from the inclusion of additional information on the i-Share cohort in the methods section. This would allow some of the information in this paragraph to be removed or clarified.

	2. Some of the demographic information presented in the text of the results is not included in Table 1. Whilst it is included in Table S1, I suggest that it if is important enough for inclusion in the text, it is important enough for Table 1. 3. It is still unclear where the results: “Self-esteem among students who declared average or poor health at 1st follow-up was lower than students who declared good or very good health (mean=26.2, 95%CI [25.4-27.0] versus mean=28.1, 95%CI [28.8-29.5], p<0.0001)” are from. Please include them in a results table if they are to remain in the text. 4. It is clear that Table S3 presents the model by which Table 2 was developed. Given the importance of Table S3 to the overall results, please consider making it a ‘main’ results table, subject to journal requirements (i.e. Table S3 becomes Table 2, Table 2 becomes Table 3). 5. Please include (N) and (%) notations in all results tables where relevant (where proportions and numbers are shown). Discussion 1. The discussion summarises and interprets the results well. 2. The authors state that “a sampling bias could have arisen since participants were mainly healthy female students thus limiting the generalization to other student populations.” I feel that the generalisability may be limited further by the sample size and characteristics. The paper explores an important area with good data, but further research is needed (ideally with a larger and more varied sample) before generalisability is considered. Instead, the authors might consider that their results provide strong justification for future studies. Other Comments 1. Some English language and grammar mistakes can be found throughout the manuscript, though readability is improved over the previous submission. a. I believe (due to their affiliations) that English is the authors second language and so politely suggest that a native English speaker might additionally edit the manuscript prior to resubmission – perhaps the journal could suggest someone?

REVIEWER	Hans Johan Breidablik Førde Medical Trust Norway
REVIEW RETURNED	23-Feb-2019

GENERAL COMMENTS	The revised manuscript has improved. It is more realistic about the limitations of the study, selection bias of active and quite healthy young women, and the possibility for reverse causation between selkf-esteem and self-rated healthh.

REVIEWER	Pavol Mikula, PhD Department of Social and Behavioral Medicine, University of Pavol Jozef Safarik, Kosice, Slovakia
REVIEW RETURNED	25-Feb-2019

GENERAL COMMENTS	Thank you for your response, I am satisfied with your reasonings. No further comments.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Oliver Huse

Institution and Country: Deakin University, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Thank you for the opportunity to review the resubmission of this paper, which describes the association between self-esteem and self-rated health in a population of college students. The authors have done well to address the comments from reviewers. The results could still do with some editing for clarity and the authors should be careful not to overstate the generalisability of their findings. Finally, the paper requires further editing for English language and grammar before it can be considered ready for publication.

Introduction

The introduction is much improved over the previous submission. As suggested above it would benefit from additional editing for language and grammar. Additionally:

1. The suggestion that self-esteem could present a specific target for interventions is not supported in the text by previous literature. I would suggest that the justification that self-rated health is a good indication general health is sufficient for the introduction (leaving discussion of interventions for the discussion).

As advised, we have removed information about interventions in the introduction (page 5 and 6).

Methods

Again, the authors have incorporated many of the comments into the methods.

1. The paper would still benefit from additional information regarding the i-Share study. In both the text and on the timeline it would be good to know the dates of each time point and the number of participants at each time point (for the i-Share cohort, not this study).

a. For example, it seems that neither the methods or results section provide details on follow-up time (but the discussion does, which can be confusing).

Regarding the dates, the precise date for each participants cannot be provided since these dates were specific for each participant corresponding to the first anniversary of the date of the baseline questionnaire (that, as we mentioned in the methods, occurred in the period between February 2013

and April 2015). We have added this information in the method paragraph (see page 7) and have simplified the writing in the results section (see page 12). We have also added the number of participants who completed the 1st follow-up questionnaire corresponding to the 18% response rate at one year (see page 7).

We have also modified figure 1 which presents the timeline and adds information about theoretical delays between each time point.

Results

1. Paragraph one of the results section would benefit from the inclusion of additional information on the i-Share cohort in the methods section. This would allow some of the information in this paragraph to be removed or clarified.

We have now added additional information regarding the i-Share cohort (see answer to the previous comment).

2. Some of the demographic information presented in the text of the results is not included in Table 1. Whilst it is included in Table S1, I suggest that it if is important enough for inclusion in the text, it is important enough for Table 1.

Table 1 is now the complete version of the baseline characteristics initially presented in Table S1.

3. It is still unclear where the results: “Self-esteem among students who declared average or poor health at 1st follow-up was lower than students who declared good or very good health (mean=26.2, 95%CI [25.4-27.0] versus mean=28.1, 95%CI [28.8-29.5], p<0.0001)” are from. Please include them in a results table if they are to remain in the text.

These results are the univariate comparison of mean scores of self-esteem between students who declared average to poor health and students who declared good or very good health. We thank the reviewer for pointing out this sentence that was not really useful. We have then deleted this sentence and have kept only information about the multivariate model that is more valid and corresponds to the principal analysis (see page 13).

4. It is clear that Table S3 presents the model by which Table 2 was developed. Given the importance of Table S3 to the overall results, please consider making it a ‘main’ results table, subject to journal requirements (i.e. Table S3 becomes Table 2, Table 2 becomes Table 3).

As recommended, table 2 is now the complete version of the multivariable analysis. table 2 becomes table 3 and table 3 becomes table 4.

5. Please include (N) and (%) notations in all results tables where relevant (where proportions and numbers are shown).

N and % notations are now presents in Table 1 and Table 3 where relevant.

Discussion

1. The discussion summarises and interprets the results well.

2. The authors state that “a sampling bias could have arisen since participants were mainly healthy female students thus limiting the generalization to other student populations.” I feel that the generalisability may be limited further by the sample size and characteristics. The paper explores an important area with good data, but further research is needed (ideally with a larger and more varied sample) before generalisability is considered. Instead, the authors might consider that their results provide strong justification for future studies.

The generalization is now less central in the discussion of the sampling bias and we state that further research with a larger sample is needed (see page 15).

Other Comments

1. Some English language and grammar mistakes can be found throughout the manuscript, though readability is improved over the previous submission.

a. I believe (due to their affiliations) that English is the authors second language and so politely suggest that a native English speaker might additionally edit the manuscript prior to resubmission – perhaps the journal could suggest someone?

A native English speaker specialized in scientific writing has reviewed the final manuscript.

Reviewer: 2

Reviewer Name: Hans Johan Breidablik

Institution and Country: Førde Medical Trust - Norway

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

The revised manuscript has improved. It is more realistic about the limitations of the study, selection bias of active and quite healthy young women, and the possibility for reverse causation between selkf-esteem and self-rated healthh.

We thank the reviewer for the comments.

Reviewer: 3

Reviewer Name: Pavol Mikula, PhD

Institution and Country: Department of Social and Behavioral Medicine, University of

Pavol Jozef Safarik, Kosice, Slovakia

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below Thank you for your response, I am satisfied with your reasonings. No further comments.

We thank the reviewer for the comments.